xxxxxxxxxxxxxxx
xxxxxxxxxxxxxxx

Department of Medical Genetics

May 12, 1957

Dr. Arthur Kornberg
Department of Microbiology
Washington University
St. Louis Mo.

Dear Art:

We had a very pleasant visit from Dale Kaiser during the past couple of days. He certainly has a splendid story on the genetics of lambda immune specificity. You have to be congratulated on the group you have assembled.

What prompts this letter was Dale's remark that you have been pressing for a Department of Genetics in the medical school. I thought you might be encouraged to learn that precisely that has been established here— the enclosed clippings tell part of the story; the most important theme is that we have the same mission in relation to ~~Agriculture as~~ Medicine as Genetics has had hitherto to Agriculture, namely as a basic research group on which applications fostered in collaboration with the clinical departments will ultimately rest. Needless to say, we keep the closest possible relationships with the existing department, not only in spirit, but in joint appointments and other formal ties. We've all felt that Wisconsin had a unique opportunity on account of the physical proximity of the campuses, and the strength of the existing genetics group. I will be sorry if other schools follow with too half-hearted attempts, or if they put too much emphasis on human genetics, (as if you would have to work on human DNA to justify your place in the medical school.)

Another organizational approach that seems entirely feasible to me would be departments of Biochemistry & Genetics, especially in view of the speed with which people like yourself are making them converge. But again if the Genetics is not to be submerged, it should be gotten off to a strong fundamental start, and a formal recognition of its status in, e.g., the name of the group.

Needless to say, this shift of affiliation will have no marked bearing on the direction of my own research, though it promises a very substantial improvement in our lab. facilities in a year or two when we move to a new research wing to be built. It has also been developing quite independently of any of the several bids to move that have been distracting us for the past several months, one of which I might say privately has not been excluded (not Stanford, Harvard or Philadelphia). The kind of organizational work that has been needed here is hardly my meat, but the peak load is over.

You may be interested in some of the other enclosures, which are self-explanatory. I hope you are going to be able to sit in at the symposium, though it might possibly conflict with the Fed. meetings.

In the lab., my own principal pre-occupation has been with the protoplasts and L-forms of E. coli, which have been fairly well worked out now in certain aspects, especially their equivalence. A number of 'permanent L-forms' of E. coli K-12 have been isolated and proved to be auxotrophs for diaminopimelic acid, which is a unique constituent of bacterialwalls, and by the same token, absent from conventional media. Other L forms (esp. in Proteus have not been reparable--the same applied for the penicillin inhibition-- and may be blocked at a polymeric step of wall formation.) We are looking now for other classes of relevant auxotrophs, esp. for D-amino acids and glucosamine derivatives, mainly just to round out the story. The protoplasts can grow in agar by bleb-formation, the restraints of the semi-solid medium partly replacing the function of the normal wall; in liquid medium, the protoplasts can only expand, as they grow, until they eventually burst.

But the main reason I was interested in this angle from the start was the hope of using protoplasts for dna-transduction. This has been a dismal failure so far, but I have talked myself into believing my own remarks at the Baltimore symposium, and am making a major push in searching for such a system in E. coli strains, by screening a great many strains, and by trying a variety of environmental conditions. So far, no soap, except for finding that a few wild-type coli's are remarkably mutable for a variety of markers. You can be sure I'll be in touch with you pdq if anything breaks. I'd appreciate hearing how you're getting along--at the very least, getting your papers on DNA synthesis as they come out . We've been using fairly crude extracts, e.g. from protoplasts, or rather sloppy DNA preps. so far. If you have a favorite procedure for pulling out 'fresh' DNA from coli, I'd be glad to see it.

With best regards,

Fraternally yours,

Joshua Lederberg